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ROYAL ASTRONOMICAL SOCIETY.

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Dr. J. W. L. GLAISHER, M.A., F.R.S., PRESIDENT, in the Chair.

Alexander Eugen Conrady, F.R.M.S., 104 Park Street, Camden Town, N.W. ;
Henry Tresawna Gerrans, M.A., Worcester College, Oxford ; and
Maurice Edmond Gheury, 23 Downshire Hill, Hampstead, N.W.,

were balloted for and duly elected Fellows of the Society.

The following Candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended :—

Henry Cunliffe Hardman, Ruskin College, Oxford (proposed by Sir R. S. Ball) ;
Francis Kennedy McClean, 1 Onslow Gardens, South Kensington, S.W. (proposed by Frank McClean) ; and
Rev. Edwin Albert Phillips, B.A., Lecturer in Mathematics, the Training College, Exeter (proposed by Arthur W. Clayden)

Seventy-two presents were announced as having been received since the last meeting, including amongst others :—

Sir N. Lockyer, Total Eclipse of the Sun, May 28, 1900 : Account of Observations, presented by the Solar Physics Committee ; 63 Charts of the Photographic Chart of the Heavens, presented by the French Ministry of Public Instruction.

Jacobi's Nome (q) in Astronomical Formulæ with Numerical Tables. By R. T. A. Innes.

The Nome q, introduced by Jacobi, can be used in several astronomical formulæ, chiefly as a convenient means of calculating the values of the complete elliptic integrals. It is defined by the following equations :—

$$q = \varepsilon^{-\pi\frac{K_1}{K}}$$

wherein

$$K = \frac{2}{\pi}\int_0^{\frac{\pi}{2}} \frac{d\chi}{\sqrt{1-k^2\sin^2\chi}}; \qquad K_1 = \frac{2}{\pi}\int_0^{\frac{\pi}{2}} \frac{d\chi}{\sqrt{1-k_1^2\sin^2\chi}}$$

and

$$k^2 + k_1^2 = 1.$$

We also have

$$K = 1 + \left(\frac{1}{2}\right)^2 k^2 + \left(\frac{1\cdot 3}{2\cdot 4}\right)^2 k^4 + \left(\frac{1\cdot 3\cdot 5}{2\cdot 4\cdot 6}\right)^2 k^6 + \&c.$$
$$= [1 + 2q + 2q^4 + 2q^9 + 2q^{16} + \&c.]^2$$

From these formulæ we may derive

$$q = \tfrac{1}{16}k^2 + \tfrac{1}{32}k^4 + \tfrac{21}{1024}k^6 + \tfrac{31}{2048}k^8 + \&c. \quad \ldots \quad \ldots \ (a)$$
$$q^{\frac{1}{2}} = \tfrac{1}{2}\sqrt{k} + 2(\tfrac{1}{2}\sqrt{k})^5 + 15(\tfrac{1}{2}\sqrt{k})^9 + 150(\tfrac{1}{2}\sqrt{k})^{13} + \&c. \ \ldots \ (b)$$

Schwarz ("Formeln und Lehrsätze zum Gebrauche der elliptischen Functionen," Art. 40) remarks that the nome q and every power of that quantity of which the exponent is a positive number, may be represented by series proceeding according to powers of k, the coefficients of the different terms of these series are positive numbers, and their sum in each case is equal to unity. Unless k is very small the convergency of the two series (a) and (b) leaves something to be desired.

For practical work the series :—

$$q = \tfrac{1}{2}l + 2(\tfrac{1}{2}l)^5 + 15(\tfrac{1}{2}l)^9 + 150(\tfrac{1}{2}l)^{13} + \&c.$$

where

$$l = \frac{1-\sqrt{k_1}}{1+\sqrt{k_1}},$$

or if $k = \sin\theta$

$$l = \tfrac{1}{2}\frac{\sin^2\frac{\theta}{2}}{(1+\sqrt{\cos\theta})^2}$$

is very convergent.

As K_1 bears the same relation to k_1 that K does to k, it is

evident that if q is tabulated as a function of θ, it will not be necessary to extend the table beyond 45°.

Let q_1 stand to k_1 as q does to k, then

$$q = \epsilon^{-\pi \frac{K_1}{K}} \qquad \text{and} \qquad q_1 = \epsilon^{-\pi \frac{K}{K_1}}$$

and

$$\log \frac{1}{q} = \pi \frac{K_1}{K} \log \epsilon \; ; \qquad \log \frac{1}{q_1} = \pi \frac{K}{K_1} \log \epsilon$$

whence

$$\log \frac{1}{q} . \log \frac{1}{q_1} = (\pi \log \epsilon)^2$$

or by taking logarithms

$$\log \text{ of } \log \frac{1}{q} + \log \text{ of } \log \frac{1}{q_1} = 0{\cdot}269868368 \; \ldots \quad (c)$$

Several tables of q, $\log q$ and $\log$ of $\log \frac{1}{q}$ have been computed, viz. :—

1840.	Jacobi.	Log q to 5 places from 0° to 90° at intervals of 0°·1. (*Crelle's Journal*, vol. xxvi. p. 93.)
1841.	Loxhay.	Log of $\log \frac{1}{q}$ to 12 or 14 places from 0° to 45° at intervals of 0°·1. (In Verhulst's *Fonctions Elliptiques*, Bruxelles.)
1860.	Meissel.	Log q to 8 places from 0° to 90° at intervals of 0°·1. (*Sammlung mathematischer Tafeln* 1, Iserlohn.)
1870.	Bertrand.	Log q to 5 places from 0° to 90° at intervals of 5′, and of log of $\log \frac{1}{q}$. (*Calcul Intégral*, Paris.)
1877.	Glaisher.	Of q and $\log q$ to 8 places for every degree from 0° to 89°. (*Monthly Notices, R.A.S.*, vol. xxxvii. pp. 372–4.)
1879.	Schömlich.	Log q to 5 places for every degree. (*Vorlesungen der höheren Analysis*, p. 448.)
1898.	Levy.	Log q to 5 places from Bertrand *C.I.* (*Fonctions Elliptiques*, Paris.)
1901.	Hoüel.	Log q to 4 places for every centesimal degree. (*Tables Numériques*, Paris.)

(See also Glaisher, Tables, *Ency. Brit.*)

The defect of those tables of the above which have sufficient decimals * for astronomical purposes is their lack of differences, so that on each occasion the tables are used the interpolation would be a longer work than a computation of the function from one of the above formulæ.

I had therefore commenced a table to every minute of the first 45°, giving q to ten places of decimals, with $\log q$ and log of $\log \frac{1}{q}$

* I have not seen Meissel's Table.

to eight places. This work was not persevered in for several reasons. For tables of not very frequent use they would occupy considerable space. Interpolations would still require the use of second differences in the most favourable part of the table (near 45°) for log q, whilst for small angles interpolation fails altogether in the cases of log q and log of log $\frac{1}{q}$. The slow rate of change of a function

$$\frac{q}{\tan^2 \frac{\theta}{2}}$$

suggested that the tabulation of that quantity would suit all ordinary requirements.

Short specimens of the earlier computed tables are given :—

θ		q	Δ	log q.	Δ	log of log $\frac{1}{q}$.	Δ
° 0	′ 0	0·000,0000,000+	53	$\bar{\infty}$		∞	
	1	053	159	$\bar{9}$·7233,3226		0 9178,5552	
	2	212	264	$\bar{8}$·3253,9227		·8850,5619	
	3	476	370	·6775,7482		·8646,5494	
	4	846	476	$\bar{8}$ 9274 5233		·8495,7588	
	5	1,322	582	$\bar{7}$·1212,7241	Too large to be tabulated.	·8375,0811	Too large to be tabulated.
	6	1,904	687	·2796,3497		·8273,9286	
	7	2,591	794	·4135,2863		·8186,5281	
	8	3,385	899	·5295,1262		·8109,3699	
	9	4,284+	1005	·6318,1777		·8040,1548	
	10	5,289		$\bar{7}$·7233,3286		·7977,2910	
		. . . . .					
° 44	′ 50	0·042,8540,371$_{358960}$		8·6319,9174$_{36363}$		0·1360,8872$_{11545}$	
	51	8899,331$_{359163}$		23,5537	353	59,7327	5
	52	9258,494$_{359369}$		27,1890	343	58,5781	5
	53	9617,863$_{359573}$		30,8233	334	57,4236	6
	54	9977,436$_{359779}$		34,4567	324	56,2690	5
	55	3,0337,215$_{359984}$		38,0981	314	55,1145	5
	56	0697,198$_{360188}$		41,7205	304	53,9600	6
	57	1057,386$_{360394}$		45,3509	295	52,8054	5
	58	1417,780$_{360598}$		48,9804	285	51,6509	5
	59	1778,378$_{360805}$		52,6089$_{36276}$		50,4964$_{11546}$	
43	0	0·043,2139,183		8·6356,2365		0·1349,3418	

The table of log $\frac{q}{\tan^2 \frac{\theta}{2}}$ which follows was therefore computed.

Vega's 10-place tables (Italian edition) were used, hence the tenth decimal is somewhat doubtful. The calculations were checked by differencing. The higher differences in several parts of the table suggested alterations of a few units in the last decimal place of several of the functions ; these alterations were adopted, so that finally the run of the differences was smooth enough. Four orders of differences were then changed into differential coefficients and divided by 1, 2, 6 and 24 respectively. The logarithms of the resulting numbers are given. This makes the use of the table in actual practice exceedingly easy, as simple powers of the decimal portion of any given argument are only wanted. I could not derive a workable formula showing the literal forms of the differential coefficients, so that the numbers given probably lack rigid accuracy. Numerous tests, taking the argument positively and negatively (*e.g.* $23° + 6'$ and $24° - 54'$) seldom differed by more than one or two units in the last decimal place. Even so q will be furnished correctly to at least 11 places of decimals, an accuracy more than sufficient for any practical requirements.

Log q was computed from the working formula

$$\log q = \log \frac{l}{2} + 2Mq^4 - 3Mq^8 \qquad (M = 0{\cdot}434\ldots)$$

In this formula the approximate value of q required was taken from Glaisher's table already mentioned. The term $3Mq^8$ amounts to one unit in the 11th place of decimals of log q at 45°. The total correction to the approximate formula

$$\log q = \log \frac{l}{2}$$

is shown in a table at the end of this paper. It is arranged so that to seven places of decimals no interpolation is necessary.

Auxiliary Table for the Computation of log q.

$$f(\theta) = \log\left[\frac{q}{\left(\tan\frac{\theta}{2}\right)^2}\right]$$

θ	$f(\theta)$.	$\log f_1(\theta)$.	$\log \frac{1}{2} f^{II}(\theta)$.	$\log \frac{1}{6} f^{III}(\theta)$.	$\log \frac{1}{24} f^{IV}(\theta)$.
0°	9·3979400,087	$-\infty$	$-\infty$	$-\infty$	0·77
1	00,093	1·3979	1·5798	1·3979	·78
2	00,187	2·3032	2·1790	1·7076	·79
3	00,597	2·8325	2·5328	1·8808	·80
4	01,701	3·2087	2·7832	2·0004	·81
5	04,033	3·4998	2·9777	2·1038	·82
6	08,278	3·7380	3·1374	2·1875	·83
7	15,281	3·9397	3·2728	2·2577	·84
8	26,046	4·1147	3·3906	2·3181	·85
9	9·3979441,741	4·2693	3·4947	2·3729	0·86

θ	$f(\theta)$.	$\log f^{I}(\theta)$.	$\log \frac{1}{2} f^{II}(\theta)$.	$\log \frac{1}{8} f^{III}(\theta)$.	$\log \frac{1}{4} f^{IV}(\theta)$.
10°	9·3979463,698	4·40782	3·5884	2·4232	0·87
11	79493,421	4·53340	3·6735	2·4698	·88
12	79532,589	4·64831	3·7516	2·5119	·90
13	79583,061	4·75426	3·8239	2·5527	·91
14	79646,882	4·85264	3·89137	2·5911	·92
15	79726,293	4·944492	3·95453	2·6274	·93
16	79823,733	5·030677	4·01406	2·6618	·95
17	79941,848	5·111897	4·07041	2·6955	0·97
18	80083,503	5·188749	4·12405	2·7284	1·00
19	9·3980251,790	5·261719	4·17522	2·7597	1·02
20	9·3980450,038	5·331227	4·22425	2·7903	1·04
21	0681,826	5·397617	4·27131	2·8209	·06
22	0950,991	5·461199	4·31666	2·8506	·08
23	1261,647	5·522241	4·36048	2·8797	·10
24	1618,196	5·580968	4·40290	2·9090	·12
25	2025,345	5·637588	4·44406	2·9375	·15
26	2488,124	5·692279	4·48411	2·9661	·18
27	3011,906	5·7452029	4·52313	2·9943	·20
28	3602,426	5·7965022	4·56126	3·0224	·23
29	9·3984265,805	5·8463056	4·59852	3·0504	1·26
30	9·3985008,569	5·8947253	4·63502	3·0785	1·28
31	85837,679	5·9418686	4·67087	3·1065	·31
32	86760,563	5·9878312	4·70609	3·1345	·34
33	87785,144	6·0326993	4·74076	3·1626	·38
34	88919,872	6·0765523	4·77493	3·1909	·40
35	90173,764	6·1194634	4·80866	3·2193	·43
36	91556,443	6·1615007	4·84201	3·2473	·46
37	93078,189	6·2027275	4·87501	3·2765	·48
38	94749,979	6·2432005	4·90770	3·3056	·53
39	9·3996583,544	6·2829758	4·94013	3·3349	1·56
40	9·3998591,427	6·3221033	4·972342	3·3642	1·59
41	9·4000787,048	6·3606305	5·004360	3·3939	·63
42	3184,773	6·3986027	5·036233	3·4241	·66
43	5799,995	6·4360634	5·068004	3·4547	·70
44	8649,221	6·4730529	5·099701	3·4856	·74
45	9·4011750,169	6·5096105	5·131346	3·5169	1·79

$$\log \text{ of } \log \frac{1}{q^{1}} = 0{\cdot}2698683{,}680 - \log \text{ of } \log \frac{1}{q}.$$

To find q we have

$$\log q = \log \tan^2 \frac{\theta + h}{2} + f(\theta) + h f^{I}(\theta) + h^2 \tfrac{1}{2} f^{II}(\theta) + h^3 \tfrac{1}{3} f^{III}(\theta) + h^4 \tfrac{1}{4} f^{IV}(\theta).$$

As an example let us compute $\log q$ for $\theta = 44^\circ\ 4'\ 18''{\cdot}31$ (or $44^\circ + {\cdot}071753$ or $45^\circ - {\cdot}928247$).

Table 44° =		9·400,8649,221.	45° =	9·401,1750,169
1st diff.	= +	213,252.	= −	3001,055
2nd „	= +	648.	= +	116,593
3rd „	= +	1.	= −	2,630
4th „	= +	0.	= +	46
		9·400,8863,122.		9·400,8863,123

and $\log \tan^2 \frac{\theta}{2} = 9{\cdot}214,3839$ gives $\log q$ 8·615,2702.

A recent interesting application of the nome to astronomical formulæ has been made by Professor G. W. Hill in the secular perturbations of the planets (*Amer. Journ. Mathem.* vol. xxiii. No. 4, and *Astron. Journ.* No. 511). Professor Hill shows that the secular perturbations depend on two quantities, $\mathfrak{K}$ and $\mathfrak{L}$, which have the following values in the notation of this paper :

$$\left.\begin{aligned} \mathfrak{K} &= \frac{K}{k_1^2} = \frac{1}{k_1^2}(1 + 2q + 2q^4 + \text{ \&c.})^2 \\ \mathfrak{L} &= \frac{K - E}{k^2 k_1^2} = \frac{8(q - 4q^4 + 9q^9 - \text{\&c.})}{(Kk_1)^{\frac{2}{3}} k^2} \end{aligned}\right\} \quad \ldots \quad \ldots \quad (A)$$

In these formulæ, if θ $(= \sin^{-1} k)$ is under 45°, q^9 will not affect the 11th decimal, and may be neglected without appreciable error.

When θ exceeds 45°, q increases in value, and its 9th power may become sensible. If so, it may be better to avoid going beyond 45° by the following device.

From

$$q = \epsilon^{-\pi \frac{K_1}{K}}$$

we have

$$K_1 = K \frac{\log \frac{1}{q}}{\pi \log \epsilon} \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (B)$$

and from

$$E_1 K + E K_1 - K K_1 = \frac{2}{\pi}$$

we have

$$K_1 - E_1 = \frac{1}{K}\left(EK_1 - \frac{2}{\pi}\right)$$

$$= \frac{1}{K}\left(EK\frac{\log \frac{1}{q}}{\pi \log \epsilon} - \frac{2}{\pi}\right) \quad \ldots \quad \ldots \quad (B_1)$$

where

$$EK = \frac{1 + 3^2q^2 + 5^2q^6 + \&c.}{1 + q^2 + q^6 + \&c.}$$

$$\left(\log \frac{1}{\pi \log \epsilon} = 9{\cdot}86506582,\ \frac{2}{\pi} = 0{\cdot}63661977\right).$$

To apply the above formulæ to the example given by Professor Hill (*Astron. Journ.* No. 512), we have $\theta = 45^\circ\ 55'\ 41''{\cdot}69$ or $90^\circ - 44^\circ\ 4'\ 18''{\cdot}31$. Log q has already been computed for the latter angle ($44^\circ\ 4'\ 18''{\cdot}31$) and by equation ($c$), or by the formula at the bottom of the table, p. 498, we get log q for $\theta = 8{\cdot}6556778$.

In this case to compute $\mathfrak{k}$ and $\mathfrak{L}$ it would obviously be shorter to use the set of formulæ (A), but to exhibit the use of the alternative formulæ (B and B_1) they are employed.

		log of log $\frac{1}{q}$	$= 0{\cdot}1413650$
$1 + 2q$	$= 1{\cdot}082470806$	log $\frac{1}{\pi \log \epsilon}$	$= 9{\cdot}8650658$
$2q^4$	$= \quad 5782$		
$\sqrt{K}$	$= 1{\cdot}082476588$		$0{\cdot}0064308 = (\alpha)$

hence

log K	$= 0{\cdot}0688370$
$1/k^2$	$0{\cdot}3153325$
(α)	$0{\cdot}0064308$
log $\mathfrak{k}$	$0{\cdot}3906003$

Then

$$EK = \frac{1{\cdot}015303347}{1{\cdot}001700363}$$

$$\frac{EK \log \frac{1}{q}}{\pi \log \epsilon} = 1{\cdot}0287003$$

and finally

$$\log \mathfrak{L} = 0{\cdot}1270542$$

It should be noted that Professor Hill's expression for $\mathfrak{L}$ as given in the *Astron. Journ.* is approximate; but as the error is of the order q^9 it is generally negligible.

Several applications of the nome occur in Gyldén's works ; one of these is the subject of Dr. Glaisher's paper in the *Monthly Notices*.

As is well known, the elliptic integrals can be used in computing the $b^{(i)}_{\frac{1}{2}}$ coefficients in the development of the perturbative function, thus :

$$b^{(0)}_{\frac{1}{2}} = 2\mathrm{K}. \qquad (a = k)$$

$$b^{(1)}_{\frac{1}{2}} = \frac{2(\mathrm{K}-\mathrm{E})}{k} = \frac{16(q - 4q^4 + 9q^9 - \&c.)}{\mathrm{K}^{\frac{3}{2}} k\, k_1}$$

$$a\frac{db^{(0)}_{\frac{1}{2}}}{da} = \frac{2(\mathrm{E} - k_1{}^2\mathrm{K}}{k_1{}^2} = \frac{16(q + 4q^4 + 9q^9 + \&c.)}{\mathrm{K}^{\frac{3}{2}} k_1{}^2}$$

With the major planets $\theta = \sin^{-1} a$ only exceeds 45° in the case of *Venus* and the Earth, and then only slightly ($\theta = 46°\ 19'\ 49''{\cdot}04$ after Le Verrier). For the latter angle, q is 0·04616097, so that q^9 is entirely negligible. If the ratio a gave a very large angle for θ, the formulæ already quoted can be adapted so that the computation may be made with a value of q for $90° - \theta$. It is somewhat doubtful if the use of the nome in this connection, except in a few special cases, would be found so useful as the Bruns-Harzer method of mechanical quadratures. (See *Die Säcularen Veränderungen der Bahnen der grossen Planeten*, by Paul Harzer, p. 60, also a paper by the same author in the *Mém. de l'Acad. Imp. de St.-Pétersbourg*, 1886, p. 28).

Applications to Geodetical Astronomy will be found in Halphen, *Fonctions Elliptiques*, vol. ii., and in Hoüel, *Tables Numériques*.

Incidentally the following table of log q and log of log $\frac{1}{q}$ each to 10 places was prepared. Log q is new, but log of log $\frac{1}{q}$ has been taken from Loxhay's table ; it is therefore possible that there is a want of rigid correspondence between the two sets of numbers, as the values of log q have not been corrected in their last decimals by differencing.

Table of log q and log of log $\frac{1}{q}$.

θ.	Log q.	Log of log $\frac{1}{q}$.	θ.	Log q.	Log of log $\frac{1}{q}$.
°			°		
0	$-\infty$		5	6·6781266244	5213830738
1	5·2796568024	0·6739735756	6	6·8367323424	5001359410
2	5·8817829559	6147092317	7	6·9709136917	4813116479
3	6·2340757853	5758715761	8	7·0872300618	4643061837
4	6·4841077801	5460355533	9	7·1899124762	0·4487198467

θ.	Log q.	Log of log $\frac{1}{q}$.	θ.	Log q.	Log of log $\frac{1}{q}$.
°			°		
10	7·2818499344	0·4342734299	28	8·1919023594	2572218795
11	7·3651031297	4207636216	29	8·2237426822	0·2495058801
12	7·4411936923	4080374126			
13	7·5112772567	3959765166	30	8·2546057618	0·2418935379
14	7·5762522110	3844874259	31	8·2845603835	2343754355
15	7·6368308344	3734948114	32	8·3136689325	2269428411
16	7·6935874304	3629369962	33	8·3419881948	2195876184
17	7·7469918430	3527627640	34	8·3695700358	2123021482
18	7·7974333790	3429290534	35	8·3964619813	2050792612
19	7·8452382508	0·3333992574	36	8·4227077213	1979121773
			37	8·4483475420	1907944537
			38	8·4734187025	1837199374
20	7·8906825461	0·3241419463	39	8·4979557644	0·1766827230
21	7·9340020238	3151298923			
22	7·9753996364	3063393105	40	8·5219908792	0·1696771141
23	8·0150513698	0·2977492718	41	8·5455540438	1626975885
24	8·0531108519	2893412243	42	8·5686733251	1557387650
25	8·0897130092	2810986181	43	8·5913750603	1487953733
26	8·1249770129	2730065964	44	8·6136840351	1418622246
27	8·1590086662	2650517442	45	8·6356236462	0·1349341840

Table showing Error of the Assumption that $\log q = \log \frac{l}{2}$.

[In units of the 7th place of decimals,]

θ.	$\log q = \log \frac{l}{2} +$.	θ.	$\log q = \log \frac{l}{2} +$.
°		°	
0 to 11	0·000	31	1·194
12	·001	31·5	1·366
13	·001	32	1·561
14	·002	32·5	1·781
15	·003	33	2·027
16	·005	33·5	2 306
17	·008	34	2·613
18	·013	34·5	2·960
19	·021	35	3·347
20	·032	35·5	3·780
21	·047	36	4·262
22	·069	36·5	4·800
23	·100	37	5·398
24	·142	37·5	6·063
25	·199	38	6·800
26	·275	38·5	7·618
27	·376	39	8·524
28	·509	39·5	9·527
29	·682	40	10 636
30	0·906		

θ.	$\log q = \log \frac{l}{2} +$.	θ.	$\log q = \log \frac{l}{2} +$.
° ′		°	
40 0	10·636	42 40	18·802
10	11·031	50	19·467
20	11·439	43 0	20·152
30	11·961	10	20·859
40	12·297	20	21·588
50	12·748	30	22·341
41 0	13·214	40	23·119
10	13·695	50	23·921
20	14·192	44 0	24·749
30	14·705	10	25·602
40	15·235	20	26·483
50	15·784	30	27·391
42 0	16·350	40	28·328
... 10	16·934	50 ...	29·294
20	17·537	45 0	30·291
30	18·159		

Reductions of Extra Meridian Observations of Planets.

By P. H. Cowell.

(*Communicated by the Astronomer Royal.*)

The following method of reducing observations of planets with the altazimuth has been brought into use at the Royal Observatory, Greenwich.

From the observation of, let us suppose, the Moon, after correcting for instrumental errors and refraction, we have the data:

(1) At time t_1 the Moon's limb was on the standard azimuth (*e.g.* prime vertical, 45° from meridian, &c.)

(2) At time t_2 the Moon's limb was at zenith distance z, after allowing for refraction.

The method aims at transforming these data into the following:—

(1) At time T the Moon's centre was on the standard azimuth as seen from the Earth's centre.

(2) At the same time T the Moon's centre, as seen from the centre of the Earth, was at an inclination Z to the direction of gravity at Greenwich.

It is clear that when this transformation has been accomplished with argument Z we can obtain the Moon's geocentric polar distance and hour-angle; and, combining the latter with T, the right ascension also.

The quantities to be calculated are $T-t_1$ and $Z-z$; the calculation of these quantities is equivalent to reducing the Moon to a star: that is to say, it corrects for the finite parallax and finite diameter.

The method is perfectly general, and is suitable for any azimuth. The computation has been assisted by forming tables for azimuths 90°, 80°, 70°, 60°, and 45°, which are alone used at Greenwich. A special case is given below as illustration; the reasoning and the formulæ apply to any azimuth, the computing to the particular observation.

Description of Term.	Formula.	Numerical Value.
Approximate azimuth motion in one lunar second	$15 \sin \Delta \cos S$	...
Correction for motion in R.A.	$\frac{601{\cdot}643-579{\cdot}682-\mu_a}{579{\cdot}682}$	+0·00336
Correction for motion in N.P.D.	$-\frac{\sin c \sin A \operatorname{cosec} \Delta}{579{\cdot}682}$	−0·001133
B	...	...
Semidiameter	$+1L, -2L$	...
Figure of Earth term ...	$+0{\cdot}003250 \sin A$	+0·003250
Curvature correction ...	$+\frac{15^2}{2\times 206265} t^2 \cos \Delta \sin c \sin A$	...
A	...	...
$T-t_1$	$\frac{A}{B} \times \frac{601{\cdot}643}{579{\cdot}682}$	...
t_1	...	...
T	...	...
t_2	...	...
$T-t_2$	...	...
Approximate zenithal motion in one sidereal second	$+15 \sin c \sin A \frac{579{\cdot}682}{601{\cdot}643}$	...
Correction for motion in R.A.	$\frac{601{\cdot}643-579{\cdot}682-\mu^a}{579{\cdot}682}$	+0·00336
Correction for motion in N.P.D.	$\frac{\cos S}{601{\cdot}643}$	+0·00125
B′	...	...
Motion in time $T-t_2$ ...	$B'(T-t_2)$	...
Semidiameter	$+UL, -LL$	...
Observed z.d., corrected for refraction	...	...
Approximate correction for parallax	$-p\rho \sin(z+\zeta)$	...
Airy correction	...	...
Z	...	...

For planets other than Moon replace 579·682

1899 April 17^d 11^h 13^m·3. Moon on Prime Vertical, West of Meridian.

c	colatitude		38° 31′ 24″
A	azimuth reckoned from south towards west	+	90°
Δ	north polar distance	=	71° 37′ 35′
S	angle at planet subtended by line joining zenith to pole.		
μ_α	motion in R.A. in 10 solar minutes ...	+	20^s·015
μ_Δ	motion in N.P.D. in 10 solar minutes ...	+	81″·14
p	parallax		54′ 16″·39

Precept.	Term.	Remarks.
...	10·7407	For other planets read "one sidereal second."
$\times 15 \sin \Delta \cos S$	+0·0361	...
$\times \mu_\Delta$	−0·0919	...
sum	10·6849	
...	+888″·82	...
$\times p$	+ 10·58	Compression $\frac{1}{300}$.
...	+ 0·74	$t = \frac{A}{B}$.
sum	+900·14	...
...	+ 87·43 (s)	...
...	10 55 27·41	...
...	10 56 54·84	...
...	10 56 44·34	...
...	+10·50	...
...	+9″·001	...
$\times 15 \sin c \sin A \frac{579·682}{601·643}$	+0·030	...
$\times \mu_\Delta$	+0·101	...
sum	+9·132	...
...	+ 1° 35′ 89″	...
...	− 14 48·82	..
...	+67 17 40·49	...
...	− 49 57·93	$\zeta = -11′\ 11″·7 \cos A$. $\log \rho = 9·999119$.
...	− 0·11	For Moon only.
sum	66 14 29·52	

by 601·643 and omit the Airy correction.

The method of the reduction is sufficiently explained by the first and second columns of the above specimen computation.

The numbers 601·643 and 579·682 that appear in the second column are the numbers of sidereal and lunar seconds respectively in 10 solar minutes.

The formulæ are easily proved; an investigation of the Airy correction is given below.

For the figure of the Earth a compression $\frac{1}{300}$ is used, and ρ is corrected for the height of the observatory above the sea-level.

The Airy correction is the excess of

$$-\sin^{-1}[\sin p(\rho\sin z+\mu)],$$

the true correction for parallax and semidiameter, over

$$-p\rho\sin z-\sin^{-1}(\mu\sin p),$$

which have been applied under the names of "approximate correction for parallax" and "semidiameter." μ here denotes the ratio of the Moon's diameter to the Earth's equatorial diameter, and must be considered negative for the upper limb. Its numerical value is 0·273. The formula is easily reduced to the form

$$\frac{206265''}{6}\sin^3 p\sin z[0\cdot78-\sin z(\pm\cdot82+\sin z)]$$

for the lower and upper limbs respectively.

On the Accuracy of Photographic Measures: Second Note.
By H. C. Plummer, M.A.

1. Since the appearance of my first note on this subject (*Monthly Notices*, vol. lxi., p. 618), in which I took occasion to examine a recent memoir by M. Lœwy, the discussion has been shared by Mr. A. R. Hinks (*Monthly Notices*, vol. lxii., p. 132). More recently M. Lœwy has published two further memoirs of considerable length (*Conférence Astrographique Internationale, Circulaire No.* 9), in which he has developed his researches in a very elaborate manner. In what follows it will be convenient to denote M. Lœwy's first memoir (*Circulaire No.* 8) and the two later memoirs by L_1, L_2, and L_3 respectively, Mr. Hinks' paper by H_1, and my own by P_1. M. Lœwy's purpose, expressed shortly, has been to construct a formula which shall give a superior limit to the probable error of a coordinate determined photographically under any circumstances. If the validity of such a formula once determined be admitted, it will be possible to deduce from it, as M. Lœwy has done, a number of practical conclusions for guidance in the conduct of actual operations. On

the other hand, it may prove more expedient to examine the practical problems which arise individually and in a more simple and direct manner. The possibility of proceeding in this way constitutes one of the vital advantages of the photographic method as compared with methods more familiar in the past, wherein the observations, once made, could not be reconsidered or repeated.

2. The principal conclusions at which I had arrived were not disputed by Mr. Hinks, who, in a desire to avoid reference to the usual theory of errors, employed a line of argument of which it seems difficult to assess the precise value. It may be pointed out, however, that even if all that involved the language of the ordinary theory of errors had been suppressed in my paper the comparative table of discordances (P_1, p. 624) would suffice to prove the main point in question. But there is one small point, in no way affecting my conclusions, on which Mr. Hinks has expressed himself at variance with me. He says (H_1, p. 136), "I do not therefore find myself in agreement with Mr. Plummer when he says: 'We may follow M. Lœwy in denoting the accidental and systematic errors by E_a and E_s respectively, and the total probable error by E_t, and admit that $E_t^2 = E_a^2 + E_s^2$.' This is indeed in obvious contradiction to his conclusion that there is a striking compensation of errors when the plate is measured in reversed positions. . . ." I believe that this criticism is due simply to a slight misconception of the policy underlying my paper. In § 5 (P_1), where the words quoted by Mr. Hinks occur, I was following M. Lœwy's views as closely as possible. There was no proof at that stage that M. Lœwy was essentially wrong. Afterwards, in its logical place, the application of the law is denied in sufficiently clear terms. Thus in § 11 (P_1, p. 625) ". . . in general it" (*i.e.* the error removed by reversing the plate) "will not obey the axioms on which the ordinary theory of errors is founded," and again in § 12 (P_1, p. 626): "Evidently we must regard as the unit of measurement, *not* a measure made in a single orientation, *but* the mean of two effected on the plate in the direct and reversed position." These statements sufficiently cover § 5 and remove my responsibility for any preliminary statement made there.

3. But though this explains the point raised by Mr. Hinks, and shows that we are really in agreement, the point may usefully be considered further, and its examination will serve as an introduction to the remarks to be made on M. Lœwy's papers. If we have an error t which must be considered as made up of two parts a and s, so that $t = a + s$, then

$$t^2 = a^2 + 2as + s^2.$$

It follows that if mean values are taken of both sides of the equation, in a purely statistical sense and with an obvious notation,

$$\epsilon_t^2 = \epsilon_a^2 + \epsilon_s^2$$

provided that (1) a and s are really independent, and (2) positive and negative values of a are equally likely to the same amount. The formula does hold then in a certain sense for errors of mean square. But if we deduce a corresponding formula for probable errors on the supposition that the probable error bears a fixed ratio to the error of mean square, the result will be not merely wrong but unmeaning, unless both the errors a and s obey certain laws too well known to make it necessary to specify them here. And even if we *define* a probable error as bearing the ratio to the error of mean square of ·6745 : 1, which would be consistent with the ordinary theory, it would still remain true that the resulting formula would have no useful meaning, and no useful deduction whatever could be derived from it. Similarly it is easy to assume a more extended formula, such as

$$\varepsilon_t^2 = \frac{\varepsilon_1^2}{m_1} + \frac{\varepsilon_2^2}{m_2} + \frac{\varepsilon_3^2}{m_3} + \ldots$$

when a number of sources of error are in question, and there is no formal difficulty in deriving a set of constants from a sufficient number of equations which are to be obtained by varying the conditions of observation. But, again, it is quite obvious that the result can bear no useful meaning unless we are prepared to subscribe to certain articles of belief as to the nature of the sources of error involved. And there is a further difficulty in practice. The probable errors in the several cases can be determined easily enough, but we can seldom ascribe to them a high degree of precision, and that for several reasons, such as the finite number of the residuals on which they are based. Consequently the more complicated the formula assumed the more will errors accumulate in the determination of the constants, until from this cause alone an absolutely fallacious result will be obtained.

4. The principles here laid down will probably not be disputed : on the contrary, they are more likely to be considered too obvious to need expression. But they are precisely those which the method of M. Lœwy's first memoir disregarded ; and in his later work the same faulty method is pushed to what appear to me extravagant lengths. Whenever a discrepancy arises in the application of the formula it is fatally easy to assume a fresh source of error and to add a term ε_r^2 multiplied by some arbitrary numerical factor. But it is difficult to be assured that the final formula is really in accordance with the laws of probability, or that it is not vitiated by a mass of accumulated error. In the absence of such assurance the results cannot command our confidence. M. Lœwy has assumed the possibility of constructing a formula which shall include all possible sources of error and meet all cases. To speak shortly, I am not prepared to admit that possibility. A too complicated formula of the kind in question seems to me to fall of its own weight.

5. My previous paper was confined to a consideration of the accuracy of the measures of photographic images, and was not concerned with the accuracy of the positions of the images themselves. Although M. Lœwy's later work is devoted to the investigation of the precision of measured coördinates in the wider sense, and to the examination of a number of important questions, I do not intend to enlarge the area of discussion. Mr. Hinks seems to go rather too far when he says (H_1, p. 137): "It is, then, of no consequence to examine elaborately the divergences from the mean of settings on a single exposure." Undoubtedly this is only a question of a preliminary character, and its solution will not carry us very far. But it is none the less an important one, and appears fundamental in M. Lœwy's method of inquiry. To put the matter as briefly as possible, the main practical conclusion of my former paper was that no essential advantage is to be gained by measuring the position of a single image in more than two orientations of the plate, and that opposite orientations (*e.g.* 0° and 180°) should be chosen. This contention was opposed to M. Lœwy's view that measures in four orientations were necessary, and was based partly on the fact that M. Lœwy's general formula did not apply to measures made in opposite orientations. From this it appeared that *one class of error* was eliminated by reversing the plate. In an Avant-propos, in which he has done me the honour to make reference (but in no sense a reply) to my criticism, M. Lœwy attributes to me the belief that "il suffit, pour tenir compte des erreurs systématiques et pour les éliminer, de faire des mesures dans deux positions de la plaque différentes de 180°." This rather overstates my view, for I said distinctly (P_1, p. 626): "I do not mean to say that the whole error, apart from what is purely accidental, can be removed by simply reversing the plate."

6. Before I leave the Avant-propos, another passage may be mentioned which seems to indicate a misapprehension in M. Lœwy's mind. Referring to his third memoir, he says: "Cette troisième recherche . . . a . . . révélé ainsi l'existence d'une nouvelle source d'erreur. Contrairement au reproche d'avoir attribué, d'après mes formules, des erreurs probables trop fortes aux résultats, il est arrivé ce qui était à redouter, que cette précision a été surestimée. Cette dernière source d'erreur provient de la déformation de la gélatine. . . . Il résulte de ce fait une inexactitude accidentelle . . . indépendante d'ailleurs de toutes les opérations de mesure." I may be wrong in supposing that this passage is suggested by my "reproche," though the context seems to support that view. But if I am not, it suggests that possibly some slight confusion of ideas may be involved. Clearly it is no answer to the question how accurate are the measures to say that inexactitudes exist which are independent of the process of measurement. As a matter of fact the passage quoted does not apply to my paper at all. My third conclusion (P_1, p. 628) was: "If M. Lœwy's estimate of the precision

of measures so made be accepted, it does not follow that it is worth while to repeat the measures as many times as he thinks necessary." The grounds on which this opinion was based are set out in § 9 (P_1, p. 623), in which the reason assigned is precisely the "systematic error," *i.e.* "l'inexactitude indépendante de toutes les opérations de mesure."

7. At the beginning of his second memoir M. Lœwy distinguishes two parts in the total error of a measure, the accidental and the systematic, and subdivides the latter into (1) ε_{s_1} the error which he calls "l'erreur permanente physiologique," and (2) ε_{s_2}, the error which "provient d'une appréciation erronée du centre du disque." He seems to consider ε_{s_1} to be of a magnitude too small to enter into the account, an opinion of which something will be said in § 12. With regard to ε_{s_2} he says (L_2, p. 11): "je considère les deux valeurs de ε_{s_2} comme étant complètement indépendantes l'une de l'autre aussi bien en signe qu'en grandeur, la personnalité de l'observateur disparaissant ici complètement." In a case when rapid reversal is possible, he admits, however, that "il peut se produire certaines compensations avantageuses d'une partie des erreurs systématiques ε_{s_1}, si toutefois celles-ci existent." Might it not be suggested that a practical inference, such as that instrumental means ought to be provided for making a rapid reversal possible if the best result is to be obtained, is perhaps of greater value than an elaborate formula based on the principle "qu'il vaut mieux toujours attribuer aux sources d'erreurs leur maximum d'effet."

8. It appears that the second memoir was practically completed before my criticism of the first was published. It is rather hard to say whether the error removed by reversing the plate belongs to the category ε_{s_1} or ε_{s_2}, but that matters little. What I hold to be beyond dispute is that in the case of the plate designated P by M. Lœwy, a class of error did exist which did not possess the quality of mutual independence, and that his formula broke down when applied in a particular instance. Is it, then, to be supposed that that formula can safely be applied to any plate *except* the very one on which it is based? And this objection is by no means met by M. Lœwy's contentions that the compensations are due to chance. In the case of a series of measures of abscissæ (L_2, p. 77) he obtains a better compensation between certain columns of residuals by making a certain artificial arrangement. It might be suggested that this effect is not wholly unconnected with the fact that the list contains only 21 stars, and these are selected "belles images" within a restricted range of magnitude. But as regards the marked compensations proved in my former paper, I cannot help thinking that M. Lœwy's attempt to attribute them to chance is by no means favourable to his own case. If the disturbing effects of chance are so serious in one case they are likely to be as great in

others, and the numbers which he has deduced from the plate P must all lie under the same suspicion. If the argument be admitted, the weight to be attached to the results of the first two memoirs is small indeed.

9. It may be remarked at this point that M. Lœwy in his first two memoirs has sought to erect a vast edifice on a singularly slender foundation—in fact, on a single plate, if I have not misunderstood him. It is useless in a matter of this kind to seek support in vague appeals to "experience." On the other hand the strength of the critical position depends on the fact that the objections which have been raised have been based on M. Lœwy's own material. Now, as to these observations, some comments must be made. M. Lœwy remarks (L_2, p. 11): "En ce qui concerne le cliché P, quelques incertitudes très légères se sont révélées au début des mesures, où les observateurs A et B n'avaient pas la sûreté d'une pratique prolongée, mais ultérieurement, pour tous les deux, l'erreur ϵ_{g_1} s'est complètement évanouie." It is unfortunate that a research which pretends to a fundamental character should have been based on the work of inexperienced measurers. Another remark of M. Lœwy's is also noteworthy (L_2, p. 47): "Je dois à cette occasion faire remarquer que l'observateur B se trouvant absent pendant plusieurs jours, quelques-unes des mesures, huit ou dix séries, ont été effectuées par l'observateur A, substitution qui . . . ne donne lieu à aucun inconvénient." This is also unfortunate, and reveals the gratuitous nature of M. Lœwy's assumption that a change of orientation with a single observer is equivalent to a change of observers. It may also help to explain the notable discontinuities which I found in my former paper (P_1, § 7)—and Mr. Hinks confirms me—to exist in the residuals of B in certain orientations. But the fact that measures actually made by A were in effect labelled as those of B stands in no need of comment.

10. One very considerable concession M. Lœwy has made. He has not only given a general formula for the probable error according to his own ideas and assumptions, but he has also given a second solution of the problem which is based on the means of measures effected in opposite orientations of the plate. He admits (L_2, p. 57) that "cette formule aboutit pour ϵ_l^{m}" (the probable error) "à des valeurs inférieures à celles que donnerait la formule (C)." But he contests its validity on two grounds: "Non seulement la seconde solution ne peut être appliquée aux opérations de mesure exécutées dans une seule orientation, mais elle donne un résultat trop faible et trop éloigné de la vérité." The latter reason is a mere statement and nothing more. The former objection is irrelevant, for my whole point is that the mean of measures in opposite orientations is to be regarded as the unit of measurement. I do not admit the possibility in general of constructing a single formula which shall represent the accuracy of single measures and those made in

properly chosen orientations, because there is no continuity between the two cases. And therefore, when M. Lœwy manipulates his second solution by the addition of numerous terms, there can be no agreement with his method of procedure, nor can the results be accepted of which he says (L_2, p. 59): "Le système de formules (G) ainsi amélioré devient maintenant applicable à tous les cas, même pour les mesures faites dans une seule orientation de la plaque." This contradicts the very hypothesis (be it right or wrong) on which the formulæ are based, and the results need not be discussed further.

11. Certain points in M. Lœwy's third memoir may now be briefly considered. His object here is to compare the measured coordinates with those deduced from the meridian observations of *étoiles de repère*, and the material for discussion is provided by measures effected on an *Eros* plate designated BI. It is not intended to go beyond the limits imposed in § 5, except to notice one point which presents itself at the end of the memoir. M. Lœwy finds that the probable discordance between abscissæ is $0''{\cdot}167$ when determined by direct comparison and $0''{\cdot}129$ when calculated *a priori* from his formula. He claims that (L_3, p. 115) "la divergence . . . n'est pas en vérité notable." Now, clearly it might be caused by a systematic error, which has been overlooked, of probable magnitude $0''{\cdot}10$. There is no need to lay any great stress on the discrepancy, which might be accounted for in several ways; though M. Lœwy seems quite inconsistent here when his previous treatment of minute differences is borne in mind. But surely he is not justified in saying: "L'accord entre les erreurs prévues et celles qui se manifestent réellement semble apporter une démonstration décisive de la rigueur des formules destinées à l'évaluation de l'erreur probable des coordonnées rectilignes tirées des clichés." This expression of unqualified satisfaction does seem somewhat out of place.

12. There is only one other passage in the third memoir which will receive criticism, but that serious criticism. M. Lœwy gives at full length tables which contain the ordinates measured in four orientations by A and B, the differences between the means for the two observers, and the mean of the eight ordinates for each star. It is pleasant to be able to acknowledge gratefully the ample and clear form in which M. Lœwy gives his material. With regard to these tables he says (L_3, p. 88): "A l'aide de ces éléments on peut aisément se convaincre que non seulement les ordonnées trouvées par les deux observateurs sont absolument identiques (abstraction faite des erreurs accidentelles), mais encore que l'erreur physiologique ε_{s_1} n'existe pas ou n'a pas de valeur appréciable ni pour l'observateur A ni pour l'observateur B." Comment on the first remark is reserved for § 14. The truth or otherwise of the second statement may be gathered from an inspection of the Table of

Personality which I have constructed by subtracting from each measure the mean for each observer, and adding the results for each orientation and dividing the sums by 43, the number of the stars. The numbers in the first column are those of the sections of Tableau III., corresponding to the six images whose ordinates were measured.

Table of Personality.

Tableau III.	A				B			
	0°.	180°.	90°.	270°.	0°.	180°.	90°.	270°.
	″	″	″	″	″	″	″	″
2	+0·092	−0·102	+0·053	−0·042	+0·070	−0·037	+0·029	−0·062
3	+ ·096	− ·117	+ ·077	− ·056	+ ·076	− ·044	+ ·020	− ·050
4	+ ·070	− ·103	+ ·073	− ·038	+ ·059	− ·019	+ ·032	− ·071
5	+ ·092	− ·095	+ ·071	− ·066	+ ·086	− ·055	+ ·050	− ·081
6	+ ·113	− ·112	+ ·083	− ·085	+ ·075	− ·046	+ ·038	− ·068
7	+ ·114	− ·120	+ ·076	− ·069	+ ·075	- ·051	+ ·038	− ·064
Means =	+ ·096	− ·108	+ ·072	− ·059	+ ·073	− ·042	+ ·035	− ·066

In the face of these figures can M. Lœwy's denial of the existence of the "erreur physiologique" be maintained? But perhaps he will say they are small. "Valeur appréciable" is a relative term, it is true. But there is an answer to this objection which seems to me final. *The magnitudes of these constant errors are in many cases sufficient practically to control the signs of the residuals.* Thus for the observer A's measures in orientation 0° *only* 40 *residuals are negative in a total of* 258! Surely so great an "écart" ought to be attributed to a cause.

13. The task of discussing a set of residuals is always a delicate one, and M. Lœwy has not avoided a serious error. The true principle was expressed admirably by Gauss, and in this matter we have not advanced beyond his position when he wrote: "Scilicet observatoris est, omnes caussas, quæ errores constantes producere valent, sedulo investigare, et vel amovere, vel saltem earum rationem et magnitudinem summo studio perscrutari, ut effectus in quavis observatione determinata assignari, adeoque hæc ab illo liberari possit, quo pacto res eodem redit, ac si error omnino non affuisset." In addition to eliminating the personal equation, there seems to be, contrary to what might have been expected, little extra advantage in the case of these measures in combining opposite orientations. But that is not really surprising. M. Lœwy has detected more pronounced errors in the measures of the fainter stars, and these have been omitted, so that we are dealing only with a limited range of magnitude. The images possess a more homogeneous character, and the measures are naturally of a higher order of accuracy. The "systematic error" with which M. Lœwy alarmed us must be very small, and further compensations cannot be expected when the errors are almost purely accidental. In practice constant per-

sonal equation matters little, for it disappears of course in the process of finding the plate constants. It amounts merely to a virtual displacement of the réseau. But it has one inconvenience. If a doubt arises as to the correctness of the measures of an individual star, it is impossible afterwards to revise the measures with any certainty unless the personality of the original measurer was determined at the time. For instance, this impossibility applies to the whole of the astrophotographic plates measured at the Paris Observatory. I regard some of the smaller numbers in the preceding table as due, not to the absence, but to the variability of the personal equation. The residuals in some cases seem to suggest this idea very strongly.

14. I have already remarked that M. Lœwy's statement that a change of orientation with a single observer is equivalent to a change of observer is simply an unproved assumption. It is interesting, therefore, to examine the differences between the means of measures by A and B of the ordinates of six images of forty-three stars. Now such differences are equally likely to be of either sign, and the chances of 6, 5, 4, 3 like signs are as 1 : 6 : 15 : 10. As a matter of fact, the numbers of sets of differences with the stated numbers of like signs are 7, 11, 11, 14 respectively, where we should expect about 1 or 2, 8, 20, and 13. The numbers are too small to lay any stress upon them. But the number of sets of differences without a change of sign seems rather larger than it ought to be. Accordingly these cases seem to be worthy of being reproduced in detail, an extra star being given which is not strictly qualified to appear. The unit of the differences is 0″·0001, and the numbers at the bottom may be regarded either as sums with this unit or as means with the unit 0″·001.

Star No.	9	15	17	27	29	36	38	25
Image 1	+ 3	+ 64	− 12	− 9	+ 12	+ 4	− 6	− 30
2	+ 1	+ 57	− 4	− 3	+ 2	+ 12	− 19	− 20
3	+ 2	+ 61	− 10	− 5	+ 80	+ 7	− 7	− 37
4	+ 14	+ 3	− 15	− 32	+ 23	+ 7	− 8	− 19
5	+ 13	+ 45	− 25	− 27	+ 7	+ 4	− 11	− 7
6	+ 8	+ 21	− 26	− 27	+ 11	+ 6	− 14	+ 4
	+041	+251	−092	−103	+135	+040	−065	−109

Now the stars 9, 36, and 38 can be dismissed from consideration as satisfactorily measured. But can the same be said of the others? Each observer has effected his measures in four orientations on six several images. Is it right that in four cases (roughly 10 per cent.) a discordance of practically 0″·1 or more should appear between the means, while in a fifth case the discordance actually amounts to a quarter of a second of arc? The odds against such a case as is presented by star 15 must be something like 10,000 to 1, if chance alone is in operation. Of

course it is possible that M. Lœwy has been unfortunate, and has included among his forty-three stars a case which will only recur once in, say, 10,000 times. If that be so, it is singularly unfortunate that all the observations of this star were not most carefully re-examined. It is curious that M. Lœwy nowhere mentions that immense advantage of the photographic over more direct methods, the possibility of easily referring again to the plate in cases of doubt and revising the measures with special care. But here I cannot think that we have to do with a mere play of chance. It does seem that the observers A and B do systematically differ in their estimation of the centre of the star, and as the difference does not appear merely in the measures of a single image a real cause must perhaps be sought in the nature of the star! This will no doubt appear fanciful,* but it cannot be denied that we have here a singular and apparently inexplicable cause for disquietude. At any rate, it seems clear that the method of multiplied orientations is *not* effective in all cases, though it may be so in the case of the majority of images. If exceptional cases be admitted, we have another reason for saying that no general formula can be constructed for the probable error of a measured coordinate.

15. In conclusion, although M. Lœwy has raised a number of interesting and important questions and has made suggestive contributions towards their solution, the questions have probably not received their final answer, and there seems to be still ample scope for further inquiry. The grounds on which this opinion is based may now be summarised thus :

(*a*) The general method adopted by M. Lœwy is open to objection for two reasons : (1) It is based on assumptions as to the nature of the errors involved the soundness of which is certainly doubtful ; (2) the results may be vitiated in a great measure by an unavoidable accumulation of arithmetical error (§§ 3, 4).

(*b*) The observational material discussed is not without defects : (1) It is too restricted in quantity and derived from too small a number of plates ; (2) some of the measures were made by inexperienced observers ; (3) insufficient care was taken to keep the results of different measurers distinct (§ 9).

(*c*) The contention that personality has no appreciable value cannot in general be maintained. The effect of this equation is proved to be in some cases pronounced (§ 12).

(*d*) The method of changing the orientations in which measures are effected may serve its purpose in reducing the systematic error of measurement in the majority of cases. But it is at least possible that exceptional cases may occur in which the multiplication of orientations will fail to reconcile estimations of the centre of an image on the part of different observers.

* Perhaps a more reasonable cause of the systematic discrepancy may be found in local irregularities in the character of the réseau lines.

An instance is quoted in which such a discrepancy persists even for different images of the same star (§ 14).

University Observatory, Oxford:
1902 *May* 7.

Visual and Spectroscopic Observations of the Sun-spot group of 1901 *May* 19–*June* 26. By the Rev. A. L. Cortie, S.J., F.R.A.S.

The group of Sun-spots of 1901 May 19–June 26, seen during two solar rotations, is noteworthy for several reasons. First, it was a large group visible to the unaided eye, occurring at a time of Sun-spot minimum. If one group of small spots seen between March 3–11 be excepted, the solar surface had been quiescent for almost exactly six months before the appearance of this group, without any spots and with very few faculæ, and these faculæ more than ordinarily faint. The following table deduced from the Stonyhurst series of Sun-spot drawings, supplemented by a very full series of direct visual observations by Mr. Hadden, of Alta, Iowa, U.S.A., shows the quiet state of the solar surface between 1900 November and 1901 April before the advent of the big group of May–June. The greatest visible disc-area of the group from the Stonyhurst drawings was 7·6 on May 22, the unit being $\frac{1}{5000}$ of the visible disc. In the former Sun-spot minimum of 1889 a big spot also appeared, that of June, as a contrast to the general quiescence. The recurrence of the phenomenon during the present minimum, even if only a coincidence, is noteworthy.

Month.	Days of Observation.	Mean Daily Spots.	Number of Groups. Faculæ.
1900 November	26	0·42	0·62
December	29	0·10	0·59
1901 January	28	0·00	0·18
February	26	0·31	0·58
March	30	0·53	0·63
April	30	0·00	0·70
May	31	0·61	1·09
June	30	0·73	0·73

1. A unique interest attaches to the 1901 outburst in that, as Mr. Perrine and the Greenwich observers have shown, it was very close in position when just out of sight beyond the E. limb on May 18, to a fine prominence, and, what is still more remarkable, to a disturbed area of the solar corona observed during the total

solar eclipse of that date. In a previous short account of this Sun-spot group communicated to the *Observatory* (No. 306, June 1901), and before any knowledge of the coincidence in position of the Sun-spot, prominence, and coronal disturbance was attainable, it was stated that "the group was born very close to the E. limb on the invisible side of the Sun, being probably two or three days old when first observed at Stonyhurst on May 20th." In the paper, "On the Types of Sun-spot Disturbances" (*Astrophysical Journal*, vol. xiii. No. 4, May 1901), it is shown how it is possible from the type of a Sun-spot to give a very fair estimate of its age and the date of its birth, even if, as in the present case, it was born on the invisible side of the Sun. It is then most likely—in fact almost certain—that the disturbance in the corona was not only coincident in position with the Sun-spot, but in time also with the date of its birth, and that we witnessed in the corona the effects of the solar disturbance accompanying the birth of the spot.

2. Premonitory signs of this, the only great outburst of the year, had been given for about a month previously to its appearance. On the drawings of April 15, 16, 17, the observer notes that regions north of the equator were disturbed as indicated by the appearance of many groups of evanescent veiled spots. On the 20th the Sun's surface displayed most active and continual changes in the appearance and disappearance of veiled spots. It is to be noted, too, from the table given above that the mean daily number of faculæ gradually increases from January to April, even though they were faint and not very extensive. On April 21 a bright patch of faculæ was both noted by Mr. Hadden and drawn by the Stonyhurst observer on the Sun's E. limb. Its character of brilliancy and compactness was such as accompanies the first appearance of Sun-spots, though on the three following days it was followed by other less brilliant faculæ of more extended area. There is a bare possibility of these faculæ indicating the region of a short-lived spot group that ran its course on the invisible side of the Sun. But the position of the bright patch of April 21 was longitude 168° and N. latitude 8°·5. This is the very position of the spot which subsequently appeared on May 19 at the next rotation, a difference of 8° in longitude between the centre of the faculous and spot groups being accounted for by the extent of the faculæ, and by the difficulty of making extremely accurate measurements when objects are close to the Sun's limb. The coordinates of the spot group on May 20 were longitude 160° and N. latitude 9°. We have here another instance of a facula preceding a Sun-spot to be added to the seven clear cases found on the Stonyhurst drawings, but in this case what is remarkable is that it precedes it by a whole solar rotation. If the barely possible surmise of the faculæ of April 21 and following days, being the remnants of a short-lived storm which had occurred on the invisible hemisphere of the Sun, be admitted, then at least we have a good instance of one spot following another in almost

the same position, and thus giving rise to a disturbed area, observed also in the solar corona of May 18.

3. During the first appearance the spot was visible from May 19 to May 31, attaining its greatest area on May 22. From May 13 to May 22 the magnets were absolutely quiet; from May 22 to May 29 there was a series of small disturbances. May 30 was a quiet day, followed by a small disturbance on May 31, so that during its first passage across the visible disc, when the spot displayed great activity and attained its maximum area, the magnets did not show even a moderate disturbance. In June the spot was visible for a second time from the 14th to the 26th, when it was much reduced in size, being almost extinct when it reached the W. limb on June 26. There were two moderate disturbances of the magnets on the 13th and 21st, the only two during the month. The other days were days of calm or but slight disturbance. As a set off there were also two moderate disturbances in July when the Sun's surface was almost absolutely quiescent. The one great solar disturbance of the year, therefore, which showed itself in a spot visible to the naked eye, in a fine prominence, in bright faculæ, and in a unique coronal disturbance, was unaccompanied by any considerable magnetic storm, and seemingly had but a fortuitous connection with the slight and moderate disturbances which occurred during its existence.

4. The spectrum of the spot was first observed on May 22 between 2 and 3·30 P.M., G.M.T., when the seeing was excellent. The Browning automatic spectroscope attached to the Perry Memorial 15-inch telescope was used for the observation, the full battery of 12 prisms of 60° being employed. Before the detailed examination of the portions of the spectrum contained in the following table was made, the whole of the lines between C and D were carefully passed in review several times. The lines usually observed as most widened were unchanged, the most widened lines being faint lines chiefly due to vanadium. The general absorption was only moderately dark.

Wave-length.	Element.	Widening.	Intensity.	Remarks.
6039·9	V	1·0	0	Most widened line.
6053·9	Ni	1·0	0	„ „ „
6063·1	...	1·0	0	„ „ „ Observed May 2
6243·1	V	1·0	000	„ „ „
6252·8	...	0·5	7	V line at 6252·0.
6254·4	Fe	0·5	1	
6254·5			5	
6256·6	Ni–Fe	0·5	6	
6258·3	Ti	0·8	2	
6258·6	V		000	

Wave-length.	Element.	Widening.	Intensity.	Remarks.
6261·3	Ti	1·0	1	A V line very near.
6265·3	Fe	0·4	5	
6267·0	...	1·0	0000	A V line at 6269·0.
6274·8	...	1·0	00	A line just before head of α band, always very widened.
6282·9	Co	0·5	2	
6291·2	Fe	0·5	4	
6293·0	V	0·5	000	
6296·2	A(O)	0·5	3	Widening perhaps due to a V line at 6296·6.
6306·0	O	1·0	2	Some other lines in the α band were somewhat widened or darkened, but this pair was quite conspicuous, especially the more refrangible line.
6307·0	O	1·0	2	
6311·7	Fe	0·5	1	
6322·9	Fe	0·5	4	
6330·3	Cr	0·5	1	A Fe line very close.
6344·4	Fe	0·5	4	
6355·2	Fe	0·5	4	
6358·9	Fe	0·5	6	
6393·8	Fe	0·5	7	A close double.
6400·2	Fe	0·5	8	,, ,, ,,
6405·9	...	0·8	00	
6408·2	Fe	0·5	5	
6411·8	Fe	0·5	7	
6415·2	...	...	1	Obliterated over spot.
6417·1	Fe?	...	1	,, ,, ,,
6420·2	Fe	0·5	4	
6421·6	Fe	0·5	7	
6431·1	Fe	0·3	5	
6432·9	Fe?	0·8	1	
6436·6	Fe?	0·8	0	
6439·3	Ca	0·5	8	
6450·0	Ca	0 5	6	
6455·8	Ca	0·8	2	
6456·6	...	0	3	
6462·8	Ca	0·5	5	
6463·0	Fe		3	
6469·4	...	0·5	2	Also darkened in spot.
6471·9	Ca	0·5	5	,, ,, ,,
6475·8	...	0·8	2	,, ,, ,,
6494·0	Ca	0·5	6	

Wave-length.	Element.	Widening.	Intensity.	Remarks.
6495·2	Fe	0·5	8	
6499·9	Ca	0·8	4	
6546·5	Ti–Fe	0·4	6	
6563·0	Hα	0	40	

The identification of the lines during the observations was made by means of Higgs's Photographic Atlas of the Normal Solar Spectrum. The wave-lengths, elements, and intensities are taken from Rowland's Preliminary Table of Solar Spectrum Wave-lengths. The widening of the lines is estimated as far as possible in terms of the normal width of the line. The observations of May 22 were revised on May 23 and 24, employing the 1st and 2nd orders of a Rutherfurd grating. The same lines were affected. The most widened line between D and *b* seemed to be 5210·6, due to titanium.

The following table gives a comparison between the average widening and average intensity of the lines of the various elements observed. In Rowland's table the intensities of the lines are reckoned from 1, a line just clearly visible on the map, to 1000 for H and K. Faint lines below intensity 1 are reckoned in order of faintness from 0 to 0000. In taking an average of the intensities of the lines of any one element observed, 0 to 0000 for the sake of comparison have been reckoned as −1 to −4.

Element.	Number of Lines Observed.	Mean Widening.	Mean Intensity.
Iron	19	0·5	+ 5·2
Calcium	7	0·6	+ 5·7
Titanium	3	0·7	+ 3·0
Vanadium	4	0·8	− 2·5
Nickel	2	0·8	+ 2·5
Cobalt	1	0·5	+ 2·0
Chromium	1	0·5	+ 1·0
Oxygen?	2	1·0	+ 2·0
Hydrogen	1	0·0	+40·0
Unknown	8	0·7	+ 0·6

Excluding the lines of unknown origin, the two doubtful lines of oxygen, and the single lines of cobalt and chromium, the mean widening is almost inversely as the mean intensity. This fact again corroborates the statement made in several previous papers (*e.g. Monthly Notices, R.A.S.* vol. xlix. No. 8, June 1889, p. 417), that at least in this portion of the spectrum the most widened lines in Sun-spots are at all periods of solar activity faint lines due principally to vanadium. In the case of iron and titanium we find in the spot-spectra the same relation between

widening and average intensity as is found by Mr. Evershed (*Memoirs R.A.S.* Appendix II. vol. liv. 1901, p. [91]) between intensity in the "flash" spectrum and intensity in the solar spectrum. Vanadium and titanium are also elements the lines of which are strong in the flash but comparatively weak in the solar spectrum, and the lines of these elements are also, as has been stated before (*Monthly Notices, R.A.S.* vol. li. No. 2, December 1890, p. 77, and vol. lviii. No. 7, May 1898), very much widened in all spots whether of the maximum or minimum epoch. It would seem to follow that the level of Sun-spots is the level of the upper more extensively diffused portion of the stratum of gas which gives the flash spectrum in eclipses. The following is a summary of the chief results which follow from the detailed study of this Sun-spot group :—

1. That the disturbed area of the solar corona of 1901 May 18 marked the time of the actual outburst of the spots.

2. The outburst occurred in a region which had been disturbed for at least a whole rotation of the Sun before the birth of the spots.

3. That in this case of the only great Sun-spot of an otherwise absolutely quiet year there is no connection between the solar storm and magnetic disturbances.

4. That the most widened lines in the spectra of Sun-spots in the red end of the spectrum are always faint lines, chiefly due to vanadium and titanium.

5. That the level of Sun-spots is the level of the upper more diffused gases which give the flash spectrum in total eclipses.

Stonyhurst College Observatory:
1902 *May* 4.

The Spectrum of Nova Persei 1901 *on and after September* 5.

By the Rev. Walter Sidgreaves, S.J.

A reproduction of the spectrum of the Nova, as photographed at Stonyhurst on August 27 and September 5, is given at p. 150 of the December number of the *Monthly Notices.* Since then forty-one photographs have been taken on twenty-eight nights, distributed as in the following list; in which the letters written after the dates have the same significations as in the wave-length tables of the paper referred to : viz. H the Hilger compound prism on the Perry-memorial Equatorial, and T the Thorp objective prism on the 4-inch finder :

September 6, H; 11, T; 14, H, T; 15, H, T; 24, H.
October 4, H, T; 6, H, T; 18, H; 19, H, T; 31, T.
November 1, H; 2, T; 14, T; 22, H, T; 23, H, T; 28, H, T.
December 5, T; 14, H, T; 18, T; 20, H, T.

January 14, H ; 15, H, T ; 28, H ; 29, T ; 30, H, T ; 31, H, T.

February 10, H ; 12, H.

All these plates show the same spectrum in its general features ; the lines remain very broad, and preserve their relative intensities. This stability of the spectrum is shown in Table I. ; in which the figures of the first column are brought forward from the December paper for comparison with those of the later dates. The measured widths of the lines are given under the letter w ; but the relative intensities have been omitted on account of the discrepancy between the relations as shown by the two spectroscopes employed for the complete spectrum.

In the fourth column, for February 10 and 12, the widths of three lines only are quoted. These could be measured without difficulty, but the edges of the rest were found to be too ill-defined. The star was very small, and the trail across the plate was necessarily too slow for satisfactory definition.

TABLE I.

Bright line spectrum of Nova Persei, 1901.

Oct. 4 & 6.		Nov. 22.		Dec. 20.		Feb. 10 & 12.	
λ	w	λ	w	λ	w	λ	w
386·8	2·5	386·8	3·2	386·8	2·6	386·8	3·0
396·8	2·3	396·8	3·0	396·8	2·6	396·9	2·8
410·1 *a*	2·1	410·2	2·2	410·1	2·6		
434·1	2·3	434·0	2·9	434·0	2·6	434·0	
436·3	3·0	436·4	3·1	436·3	2·8	436·4	
447·1	2·7						
463·9	2·5	436·9	2·8	464·0	3·2	464·2	
468·0	2·1	468·0	1·7	468·0	1·7	468·8	
472·0 *b*	2·0	472·0	2·1	472·2	2·2	472·1	
486·1	2·7	486·2	3·2	486·2	2·4	486·1	3·2
495·8	3·0	495·7	3·1	495·9	2·5	496·2	
500·7	3·0	500·6	3·1	500·7	3·1	500·7	

Remarks.

a This line was omitted in the December paper as uncertain. It has since then been easily seen and measured.

b In the same December paper the width of this line was quoted 4 $\mu\mu$ on August 24 and 27. This width covered the whole band, including outside weaker wings. On the later dates of this table only the central stronger part is entered, because there is no decided termination of the violet-side extension. The appearance is rather that of a light background to the two bands 468 and 472, reading from 467 to 474, and too strong to be reckoned as continuous spectrum.

In Tables II. and III. some of the finer details are presented for further comparisons. Here also there is a general close agreement ; but not without exceptions, which seem to be real

changes, although on account of the failing light of the star, they may be only photographic imperfections. These apparent changes are, in Table II., the relative intensities of the lines 469 and 471 in November and December; and in Table III. the reappearance of lines crossing Hγ.

Table II.

Brighter lines in region 467–474.

Aug. 27. λ μμ	Oct. 6	Nov. 22.	Dec. 20
467·3	467·5	467·3 w	467·5
468·6 *c*	468·8	469·1 w	468·8 ss
470·0	470·3 w	470·3 ?	? *d*
471·2 ss	471·3 s	471·2 w	471·3 s
472·7 s	472·9	472·7 w	472·8

Remarks.— ss = very strong. s = strong. w = weak.

c By an error of copy from the original tabulations this line went to the press for the December paper as 468·1 instead of 468·5. The figures in this column are by remeasurements, which have given 470·0 and 472·7 greater than the former measures by 0·2 and 0·3 μμ.

d The uncertainty indicated here by the ? is owing to a scratch through the position where the line might be.

Table III.

Brighter lines in region 432–438.

	Sept. 5, Oct. 6. λ μμ.	Nov. 22. λ μμ	Dec. 20. λ μμ
		432·7	432·7
Hγ		433·6	433·9
		434·4	
	435·2	435·2	435·0
436·3	435·9	436·1	
	436·6	436·5	
	437·4	437·5 ?	

The structure of the bands at 386·8 and 396·9 has remained probably quite constant. This structure was given in the December paper as four bright components. At that time there was some doubt about the fourth component, whether it was divided or not. Subsequently greatly enlarged photographs showed the *five* components of the band at 386·8 on September 5, and of both bands on October 4 and 6, and November 22. Again, on December 18, the five components were very clearly marked on the more refracted band, but the companion band at 396·9 was not strong enough to show more than four bright parts, the fourth being relatively broad and very weak. And, similarly, on the plates of January 30 and 31 of this year only three of the components can be seen in the band 386·8, the less

refracted band being much too weak to show details. All through this series of photographs the more refracted of the two bands has been the stronger, and the stronger components of each have been of shorter wave-lengths.

The band at 495·9 on December 20 contains two sharply defined brighter lines near its edges, at 495·1 and 497·0; and the same marking, near the edges, appears in Hβ and in the band 500·7. The photograph of this date was by a remarkably successful exposure in a very clear sky, and compares well with the photograph of August 27. On a careful re-examination of this plate the same structure has been found in the bands 495·9 and 500·7, but only doubtfully in Hβ.

Stonyhurst College Observatory:
1902 *May*.

Series in the Nebular Spectrum, and in the Bright-Line Spectrum of Nova Persei. By E. F. J. Love, M.A., Assistant Lecturer and Demonstrator of Natural Philosophy in the University of Melbourne.

§ 1. *Introductory.*

Soon after the spectrum of Nova *Persei* had assumed the nebular type, Lockyer * and Sidgreaves † called attention to the abnormal brightness of three of the lines, compared with that of the "chief" nebular line at λ 5007; Sidgreaves further showed that these lines were characterised, in the Nova spectrum, by identity of structure, while the other lines present had a very different appearance. The wave-lengths of these lines, as estimated by different observers, group round the values λ 4363, λ 3968 and λ 3869.

It seemed to me likely that three lines so closely resembling each other might belong to a series of the general type investigated by Kayser and Runge; I therefore made the attempt to find one, with the results detailed in this paper.

§ 2. *Deduction of Series, and Comparisons with Nebular Lines.*

Since the identification of the lines above-mentioned with nebular lines seems to be beyond dispute, I preferred to use their wave-lengths as measured in nebular spectra, since these have recently been redetermined with great exactness at the Lick Observatory ‡ by Wright, in the course of the observations on

* *Proc. Roy. Soc.* vol. lxix. p. 133; *Monthly Notices*, vol. lxii. App. p. 1.
† *Monthly Notices*, vol. lxii. p. 137.
‡ *Astrophysical Journal*, vol. xiv. p. 267.

Nova *Persei*. The values given by him are 4363·3, 3967·6 and 3868·9; these I find to be included in a series of the form

$$\lambda^{-1} = A + Bn^{-2} - Cn^{-4}$$

where λ^{-1} denotes the number of waves per centimetre, A = 25739, B = 43108, C = 1412208, and n has the values 4, 5 and 6 for the three lines. The series relation therefore exists.

Recalling the important fact that the relative intensity of these lines increases as we go towards the violet—exactly the reverse of the usual behaviour of series lines *—it seemed worth while to extend the series, and see if it included other bright nebular lines. Making the calculation I find that $n = 8$ gives the wave-length † of a line observed by Campbell ‡ in the spectrum of the *Orion* nebula, while among higher members of the series I find wave-lengths corresponding to those of a number of lines observed by Huggins § in the spectrum of the trapezium stars and surrounding parts of the *Orion* nebula. Going in the other direction, I find that $n = 3$ gives a wave-length so far down in the extreme red—it corresponds nearly to the middle of the A band in the solar spectrum—as to be extremely difficult to observe in a nebular spectrum, supposing it were present.

The observed and computed values are collected in the following table; it must be noted that—except in the case of the three lines used in deducing the formula—the wave-lengths are only given, by the several investigators, to the nearest tenth-metre.

Table.

n	λ^{-1} calcd. (waves per cm.)	λ calcd. (tenth-metres.)	λ obsd. (tenth-metres.)	Observer.	Notes.
3	13104	7631·3	...	...	Not observed.
4	22917	4363·3	4363·3	Wright.	Used to deduce the formula.
5	25204	3967·6	3967·6	,,	
6	25847	3868·9	3868·9	,,	
8	26068	3836·1	3836	Campbell.	Orion nebula.
10	26029	3841·9	3842	Huggins.	Orion nebula and trapezium.
13	25945	3854·3	3854	,,	,, ,, ,,
	If it be legitimate to include $n + \frac{1}{2}$, we obtain two other identifications.				
11·5	25985	3848·4	3848	Huggins.	Orion nebula and trapezium.
14·5	25912	3859·2	3859	,,	,, ,, ,,

* Can the positive sign of the term involving B—which is also the reverse of the usual case—be connected with this fact?

† The minimum possible wave-length for the series; proved by differentiating the equation.

‡ *Astronomy and Astrophysics*, vol. xii. p. 273.

§ *Proc. Roy. Soc.* vol. xlvi. p. 40.

The inclusion of the last two lines seems legitimate, as it introduces a symmetry in the values of n which would not otherwise exist; we may then, starting from $n = 8$, arrange the lines in two groups, the successive differences being

$$(n = 8)\begin{cases} 2,\ 1,\ 1,\ 1. & \text{First group.} \\ 2,\ 1\tfrac{1}{2},\ 1\tfrac{1}{2},\ 1\tfrac{1}{2}. & \text{Second group.} \end{cases}$$

It seems inadvisable to carry the table further; if n exceed 15, an increase of a unit in its value changes the wave-length by only $2\frac{1}{2}$ tenth-metres; if it exceed 20, we obtain values which might fit almost any line, between λ 3868 and λ 3885, not very accurately measured.

§ 3. *Remarks on the Identifications in the Table.*

1. Campbell identifies λ 3836 with β (otherwise known as Hη), but this seems to me very doubtful. At the time of his observations everyone believed λ 3968 to be Hϵ, but this is now known, from Wright's investigation, to be a mistake; Hϵ is indeed present, side by side with an "unknown" line, but it is very faint, while its unknown companion is bright; hence Hη, if present at all, should be a still fainter line, but Campbell marks λ 3836 as "bright" in his table. It is true that he also identifies another bright line, at λ 3889, with α (or Hξ); but in view of the behaviour of Hϵ this may be attributed—with at least equal probability—to helium,* which has a strong line at λ 3888·8. Moreover the differences between the wave-lengths of the ultra-violet hydrogen lines and those identified with them by Campbell are much larger than any of the differences in the case of other, and quite certain, identifications; judging from these I find it much easier to believe some of Campbell's identifications wrong than to believe that his measurements are incorrect by three and four tenth-metres.†

2. Huggins's lines form part of a cluster, distinct and fairly strong in the spectrum of the trapezium stars, and extending into that of the surrounding nebula; so far they have not been obtained from other parts of the nebula. Two of the cluster, λ 3832 and λ 3825, do not belong to the series. Another line, equal in length on the plate to those identified above,‡ and of about the same order of brightness—measured by Huggins as λ 3870—is possibly identical with λ 3869; but this seems

* The helium spectrum was not known at the time of Campbell's observations; we have since learnt that it accounts for a number of lines in the spectrum of the Orion nebula.

† In view of the above arguments I am inclined to think that the hydrogen series, beyond Hϵ, has probably not yet been detected in nebular spectra.

‡ And therefore, presumably, emitted from the same part of the nebula.

improbable, as neither the lower members of the series, nor λ 3836, are represented in the photograph.*

University of Melbourne:
1902 *March* 18.

Discovery of Comet Brooks, 1902. By William R. Brooks, D.Sc.

I have the honour to announce to the Society the discovery of a new comet.

While sweeping the eastern sky on the morning of April 14, I picked up an object in *Pegasus*, which I at once suspected to be a comet. Its position at $21^h\ 5^m$ G.M.T. was approximately R.A. $22^h\ 55^m\ 40^s$, Decl. $+29°\ 12'$. A slight haze and advancing twilight made me hesitate about making a public announcement, but a telegram was sent that morning to Harvard College Observatory of the suspected object.

The following morning was awaited with much interest, which fortunately was clear. The object, it was immediately noticed, had moved from the discovery field and was soon found about three degrees nearer the Sun. Its place on April 15, 21^h G.M.T., was R.A. $23^h\ 8^m\ 10$, Decl. $+27°\ 25'$. This gave a daily motion of about 12^m east and 2° south.

The comet is a fairly bright telescopic object. It has a very minute stellar nucleus and a short narrow tail.

Comet *Brooks*, 1902.

Smith Observatory, Geneva, N.Y., U.S.A.

* The double grouping, exhibited in § 2, may account for the absence of the other radiations from this part of the nebula, and of these from other portions where they are present.

Micrometrical Measures of Double Stars with the 17¼-in. Reflector.

By the Rev. T. E. Espin, M.A.

I.—*Stars of the Mensuræ Micrometricæ.*

In the following list Column 1 gives the number in Σ, Column 2 the approximate R.A. and Decl. for 1880, Column 3 the position angle, Column 4 the distance, Column 5 the number of nights, Column 6 the magnitudes, Column 7 the date, Column 8 notes.

Σ	R.A. 1880. Decl.	P.	D.	Nights.	Mags.	Date.	
	h m ° ′	°	″				
16	0 10·3 + 54 0	37·24	5·75	3	...	00·99	
30	20·7 + 49 19	301·20	18·25	3	7·0 8·4	99 98	Note 1.
45	32·1 + 46 18	87·25	13·43	2	7·5 9·5	00·60	Note 2.
59	41·2 + 50 47	150·87	1·79	2	...	00·38	AB motion.
		124·87	34·71	2	$c = 12·2$	00·38	AC. C and D not observed before.
		160·00	10·40	1	D 12·7	00·87	CD.
97	1 5·2 + 50 53	97·35	5·05	2	8·9 9·2	99·96	
115	15·7 + 57 30	148·61	1·15	3	7·0 7·3	01·61	Increase of distance.
268	2 21·0 + 55 0	130·97	2·66	2	6·0 8·0	01·88	
270	22·4 + 55 1	303·81	20·73	3	...	01·57	
296	36·0 + 48 43	299·74	17·40	3	4·0 10·0	00·36	θ Persei. AB note.
		223·91	...	2	...	00·07	AC.
301	39·1 + 53 26	16·34	8·05	4	...	01·00	
314	44·4 + 52 30	303·23	1·42	3	6·8 7·0	00·62	Motion in angle.
331	52·5 + 51 52	84·66	11·89	2	...	00·02	
388	3 20·1 + 50 1	206·66	3·00	4	8·2 9·0	01·91	Motion in angle.
389	20·5 + 58 57	63·42	2·56	4	...	02·07	Motion doubtful. Note.
390	20·9 + 55 2	159·34	14·96	2	5·0 10·0	99·99	C.P.M.
396	23·9 + 58 22	242·70	20·24	1	...	02·05	
398	24·6 + 57 53	330·10	9·46	1	11·0 11·0	02·08	Note.
476	53·6 + 38 20	286·46	20·56	2	7·5 8·4	99·94	Increase of angle and distance.
613	4 50·2 + 43 57	104·50	16·63	4	8·2 9·0	00·29	AB. Note.
		17·74	15·91	5	11·0	00·25	AC
619	52·1 + 50 5	122·72	5·47	3	8·0 8·3	00·35	Note.
845	6 2·4 + 48 44	353·65	7·90	3	6·0 6·5	00·42	41 Aurigæ.
918	24·4 + 52 33	327·30	4·73	2	7·0 7·3	00·09	Increase of angle.
1062	7 13·1 + 50 22	315·50	14·59	2	...	00·58	19 Lyncis. Increase of angle.
1176	58·1 + 42 19	28·30	22·29	2	8·3 9·3	00·59	

Σ	R.A. 1880. h	m	Decl. °	′	P. °	D. ″	Nights.	Mags.		Date.	
1312	9	1·8	+52	54	147·75	4·28	2	...		01·10	
1321		6·2	+53	12	65·05	18·96	2	7·1	7·5	00 08	Note.
					65·55	18·77	4	...		01·11	
					65·13	18·60	3	...		02·19	
1341		14·3	+51	7	267·44	20·69	3	8·5	8·6	01·11	
1366		27·4	+53	51	323·80	7·88	3	8·0	9·1	02·22	
1368		27·8	+53	51	220·44	21·12	2	8·3	9·6	02·21	
1402		56·8	+56	4	100·60	24·90	2	6·9	7 7	00 12	Note.
1427	10	14·7	+44	30	216·76	9·80	2	7·0	7·2	01·20	
1462		35·6	+51	25	172·28	7·77	2	7·2	9·5	01·65	
1486		47·9	+52	46	101·66	28·86	2	...		00·59	
1495		52·4	+59	35	37·77	34·10	2	6·7	8·4	01·65	
1543	11	22·6	+40	0	2·18	5·26	2	5·0	8·8	02·25	Motion.
1553		30·0	+57	48	166·42	5·00	2	7·3	7·7	02·28	
1572		40·6	+53	58	289·15	10·20	2	8·4	9·2	01·82	
1608	12	5·5	+54	5	223 06	11·91	2	7·5	7·7	01·35	
1630		13·0	+57	2	168·29	2·73	2	9·0	9·2	01·82	
1662		30·2	+57	14	234·95	19·74	2	...		01·82	Motion.
1718	13	0·2	+51	37	272 06	13·21	2	8·8	9·0	01·37	
1758		27·9	+49	45	305·37	4·47	4	7·1	7·2	00·90	Motion.
1821	14	9·2	+52	21	236·78	12·74	2	4·2	8·5	00·93	κ Boötis.
1843		20·2	+48	22	186·63	20·35	2	...		01·37	
1909		59·9	+48	7	243·56	4·74	4	7·0	7·2	01·41	Motion.
1984	15	48·0	+53	16	276·93	6·92	4	6·3	8 5	01 17	Motion.
2015	16	5·2	+45	40	158·68	3·24	3	...		01·05	AB.
					96·84	14·21	2	$c = 13·2$		01·37	AC. Not seen before, difficult.
2060		26·2	+47	32	255·28	5·34	2	8·7	8·8	01·45	
2063		28·2	+45	51	195·75	16·42	3	5·5	8·1	01·08	
2068		30·5	+47	32	255·28	5·34	2	8·7	8·8	01·45	
2072		32·2	+47	56	180·19	5·46	3	8·6	9·1	01·16	Motion.
2078		33·4	+53	10	109·54	3·48	3	5·0	5·5	01·16	17 Draconis.
					194·52	90·55	2	...		00·44	16 and 17 Draconis
2082		35·5	+49	10	91·11	23·74	4	4·5	9·4	00·71	
2130	17	2·9	+54	37	326·87	2·35	3	5·0	5·2	01·16	μ Draconis.
2138		7·7	+54	39	135·55	22·32	2	8·0	8·7	00·97	
2271		57·7	+52	51	267·31	2·37	3	6·9	7·8	00·88	
2277	18	0·0	+48	28	121·45	26·91	2	6·5	8·4	01·68	Motion.
2278		0·8	+56	26	24·22	37·04	2	7·0	7·4	00·55	AB.
					147·29	6·32	2		7·7	00·55	BC.

Σ	R.A. 1880. h m	Decl. ° ′	P. °	D. ″	Nights.	Mags.		Date.	
2323		22·2 + 58 43	357·53	3·47	2	...		01·16	Motion.
2348		31·2 + 52 16	272·23	25·42	2	5·5	7·8	01·52	
2393		41·1 + 38 12	22·70	13·56	7	7·1	9·6	00·36	Increase of distance.
2433		54·7 + 56 35	125·41	7·53	2	7·2	10·0	01·66	
2463	19	2·5 + 45 37	4·08	9·55	3	8·5	9·2	01·03	Motion.
2507		16·0 + 44 9	139·18	25·80	2	7·7	9·2	01·74	AB.
			283·80	7·14	2		10·0	01·74	BC motion? *h*'s star.
2580		41·9 + 33 27	70·90	25·59	2	...		01·17	χ Cygni, motion.
2588		45·2 + 44 4	160·86	9·90	2	...		01·72	
2609		54·2 + 37 47	24·62	2·42	3	...		01·60	
2619		57·5 + 47 56	244·61	4·05	2	8·0	8·0	01·63	AB. Note.
			300·24	11·03	3		13·7	01·64	BC.
			300·15	16·72	3		12·0	01·81	AD.
			321·09	16·48	4	...		01·73	BD.
			164·4	...	1	...		01·81	DC.
2708	20	34·1 + 38 13	27·40	18·63	2	7·0	13·5	01·60	AB, Hall's star. Note.
			330·92	27·87	3		8·5	01·59	AC.
2773	21	5·8 + 43 32	115·95	3·41	2	8·3	8·7	01·64	
2789		16·7 + 52 28	296·10	6·01	3	7·0	7·1	01·01	
2813		32·4 + 56 56	272·27	10·29	3	8·8	9·0	01·08	
2815		34·0 + 57 1	79·76	7·32	3	7·8	9·5	01·08	
2840		48·0 + 55 14	195·66	19·71	4	6·0	6·5	00·38	
2902	22	18·5 + 44 45	88·07	6·07	3	7·8	8·1	01·72	
2987	23	4·8 + 48 22	158·98	3·60	2	7·5	10·0	01·76	
3010		17·7 + 45 8	132·26	25·65	2	8·3	8·6	01·71	AB.
			103·60	29·47	2		11·5	01·71	AC not seen before.
3034		38·6 + 45 43	101·10	6·49	2	7·8	10·5	00·81	Motion?

Notes.

1 Σ 30. A Proper Motion of 0″·05 towards 265°·0 in the large star will explain the change.

2 Σ 45. A Proper Motion of 0″·064 towards 176°·0 is probable.

3 Σ 296. θ Persei. The motion during the 116 years since Herschel's observation is perfectly rectilinear. I find the following:—

A 0″·338 in the direction of 105°·6
B 0″·319 „ „ „ 100°·6

and all the observations are well represented by

Δ 16″·363 + 0″·029(*t* − 1866·0)
P 297°·162 + 0°·075(*t* − 1866·0)

The star C has no connection with the system.

389. The measures of this star are most discordant. It seems probable that so far there is little if any motion.

398. De. did not measure this star, and it has been generally neglected. It is too faint to be in the B.D. It lies $33^s.f\ 2'\ n$ of B.D + 57°, 729, which has a very faint comes (too faint to measure satisfactorily with the 17¼-in.).

AB P. 325°·46. D 6″·88. Mags. 8·0 13·5 1902·08.

Σ 613. A has a Proper Motion of 0″·047 towards 114°·7.
Σ 619. A change of 16°·7 in angle since Σ.
Σ 1321. A system like 61 Cygni P.M. A 1″·711 towards 249°·1.
B 1″·742 „ 247°·0.

The later observations show that Dunér's formula is correct for position, but makes the distance too large according to recent measures. The following gives closer agreement :

$$\Delta = 19''{\cdot}721 - 0''{\cdot}02(t - 1863{\cdot}0)$$
$$P = 55°{\cdot}69 + 0°{\cdot}24(t - 1863{\cdot}0)$$

The star C was first seen 1901 Jan. 22 : it is too faint to measure satisfactorily with the 17¼-in. A and B are both strong reddish yellow.

Σ 1402. A has apparent Proper Motion 0″·07 towards 123°·5.

Σ 2619. The star D was first seen by *h*, and has been measured by OΣ and *β*. The star C was detected by OΣ. The comites are difficult to measure with the 17¼-in. The position-angle DC was obtained by setting the wire parallel with the stars.

Σ 2708. The star B was detected by Professor Hall, and is called by him 15 mag. It was detected independently with the 17¼-in. It is too faint to measure properly, but the measures are sufficient to show that the change is due to the Proper Motion of A. The various measures show that A has a Proper Motion of 0″·26 towards 137°·7 which is in close agreement with the meridian observations.

II.—*Various Stars.*

(C. A. G. refers to stars found double in the catalogue of the *Astronomische Gesellschaft.*)

Names.	R.A. 1880 h m	Decl. ° ′	P. °	D. ″	Nights.	Mags.		Date.	Notes.
Espin 40 ...	0 5·1	+ 51 24	72·88	3·27	2	8·7	11·7	02·03	AB.
			316·84	23·50	2		10·0	02·03	AC.
Espin 42 ...	16·3	+ 52 56	191·28	10·36	3	8·3	9·3	01·90	
OΣΣ 7 ...	40·0	+ 50 26	76·08	50·59	2	7·5	7·7	01·97	
Σ 83 rej. ...	56 5·0	+ 49 40	313·50	22·09	3	7·7	10·4	00·32	*h* 311°·1.
Dawes 8 ...	1 15·3	+ 43 11	140·99	2·47	2	8·5	9·0	00·98	No change.
Σ 190 rej.	50·8	+ 40 48	75·22	24·12	2	8·2	9·2	00·03	Separating from PM.
h 2136 ...	2 21·0	+ 53 19	34·74	5·02	3	8·6	9·2	00 93	*h* 37°·1
Holmes ...	3 20·0	+ 59 30	49·94	5·40	3	8·6	10·0	01·62	
OΣΣ 37 ...	30·0	+ 44 25	95·60	41·23	2	6·5	6·8	01·08	
Espin 55 ...	56·2	+ 58 58	261·30	9·01	2	8·1	12·5	01·98	A red.
Hussey 212	4 5·0	+ 51 31	192·29	3·66	3	8·9	10·0	01·06	AC found independently.
Goodacre ...	29·0	+ 16 16	331·25	3·73	2	11·7	11·7	01·13	Note.
h 2241 ...	48·9	+ 47 49	263·14	11·59	3	9·0	9·1	00·66	
C. A. G. ...	52·7	+ 54 39	159 99	4 21	2	8·8	9·2	00·56	

Names.	R.A. 1880 h m	Decl. ° ′	P. °	D. ″	Nights	Mags.		Date.	Notes.
h 2319 ...	6 24·9	+47 52	305·88	3·58	2	9·1	10·7	00·51	AB. *h* 300°·5.
			259·40	15·81	2		13·0	00·51	AC.
OΣΣ 78 ...	38·1	+43 42	24·58	45·39	4			00·34	Change from PM.
C. A. G. ...	50·8	+57 1	174·28	6·95	2	9·1	9·2	00·09	AB.
			3·62	15·46	2		14	00·09	AC. Very difficult.
h 2399 ...	7 29·7	+57 3	66·15	12·26	2	8·7	11·7	00·09	
Hussey 224?	8 12·6	+47 47	323·10	3·80	2	8·0	11·5	01·17	Note.
C. A. G. ...	46·2	+50 21	336·90	5·44	2	8·9	9·0	01·21	
h 1176 ...	10 8·6	+58 2	318·50	8·90	2	9·0	9·2	02·29	
h 2545 ...	42·0	+55 55	111·11	18·96	3	8·8	11·2	02·27	*h* 116°·3.
OΣΣ 109 ...	11 9·1	+46 31	256·83	79·25	2	6·9	7·7	02·31	
Σ 1563 rej.	32·8	+52 50	157·10	13·54	2	8·6	10·8	02·29	
Espin 73 ...	12 4·3	+55 35	305·81	3·64	3	10·5	10·7	01·68	BC.
			20·87	31·25	3		8·2	01·67	AB.
h 2617 ...	34·9	+40 56	3·61	6·24	2	8·1	8·8	02·31	Motion.
h 2627 ...	52·2	+48 7	132·15	17·77	2	8·8	10·9	01·14	
Holmes ...	13 26·8	+36 56	163·43	7·30	2	9·1	9·5	01·84	
h 1234 ...	29·3	+39 24	23·38	33·36	3	7·5	11·3	02·31	
C. A. G. ...	30·8	+50 16	14·09	3·30	3	8·9	9·1	00·41	
			9·19	3·17	4			02·30	
h 2700 ...	14 0·8	+40 33	216·23	22·33	2	8·7	12·7	00·89	
H*h* 435 ...	3·8	+50 2	274·20	81·15	2	4·7	11·0	02·29	13 Boötis. Note.
h 540 ...	5·4	+36 23	209·84	9·04		8·8	9·1	02·32	Note.
Σ I. 26 ...	11·9	+51 55	33·41	38·12	2			01·40	ι Boötis.
Espin 19 ...	16·2	+52 13	41·66	1·62	3	9·2	10·5	02·32	AB. Very difficult.
			170·38	41·91	3		9·3	02·32	AC.
h 2716 ...	18·4	+46 55	85·88	5·02	2	9·1	9·3	02·28	
h 2729 ...	26·3	+56 39	56·15	27·52	2	8·7	11·0	01·36	
Sh 191 ...	56·0	+54 21	343·15	40·00	3	6·6	7·0	00·75	
OΣΣ 137 ...	15 12·2	+51 23	105·82	74·14	2	6·1	8·6	00·50	
C. A. G. ...	16 0·6	+49 17	254·81	8·19	3	9·3	9·4	00·38	
Ho 412 ...	17 3·8	+36 6	140·42	19·78	2	5·0	11·2	00·56	
Σ 2209 rej.	39·2	+43 13	128·06	29·17	2	7·7	9·7	00·66	
h 1354 ...	18 48·8	+36 17	5·97	9·87	2	8·8	8·9	00·54	*h* 187°·0.
H*h* 603 ...	19 1·7	+35 42	54·95	45·52	1	7·5	8·7	00·57	
Aitken 152.	7·4	+36 49	257·00	2·71	3	9·1	9·2	01·58	AB. Note.
			175·00	20·67	3		10·3	01·58	AC.

Names.	R.A. 1880 h m	Decl. ° ′	P. °	D. ″	Nights.	Mags.		Date.	Notes.
H*h* 605 ...	9·4	+38 50	57·07	39·35	2	7·5	7·8	00·53	
Aitken 157	12·0	+37 10	142·74	2·13	3	9·0	10·0	01·58	Note.
Aitken 158	12·5	+38 58	292·53	3·50	3	8·4	9·7	01·58	Note.
h 1506 ...	20 18·6	+35 18	202·27	9·02	2	8·3	10·5	00·66	AB. *h* 199°·5.
			192·55	32·67	1		10·3	00·66	AC. *h* 191°·1.
C. A. G. ...	41·1	+36 20	214·25	6·30	3	9·1	9·2	00·67	
S 786 ...	21 15·8	+52 33	300·40	47·18	3	7·6	9·0	01·01	
h 1709 ...	54·0	+56 1	316·76	9·20	2	8·9	12·0	00·66	*h* 312°·9.
H*h* 764 ...	22 24·1	+57 38	9·81	25·03	2			99·86	P δ Cephei.
C. A. G. ...	27·6	+54 35	333·79	2·86	3	8·7	9·1	01·03	
Dembowski	29·4	+49 46	348·42	7·88	4	8·3	8·4	01·07	
h 1849 ...	23 2·2	+45 43	347·59	47·54	2	4·7	11·0	01·82	4 Andromedæ.
OΣΣ 248 ...	40·8	+50 0	339·40	23·23	1	6·5	12·0	01·83	AB not seen before.
			140·15	53·24	1		9·0	01·83	AC.
Holmes ...	52·4	+57 0	75·86	18·61	2	8·0	11·0	01·92	
Espin 112	53·1	+52 49	221·80	1·33	4	9·0	9·2	01·99	
OΣΣ 254 ...	55·0	+59 43	269·45	58 95	1	7·0	8·5	00·86	

Notes.

Goodacre. A little pair 62 sec. *p* α Tauri. Place of α Tauri.

Hussey 224. Found independently, and measured before Hussey's results reached me. Hussey makes the star *p* the double, and my angle differs by 10°. It is a very difficult object.

H*h* 435. I have failed to find any comes in H's place (82°·6).

h 540. *h* gives (*h*M*m* 312) 1828·33 30°·18, 14″·19, but in his second catalogue the distance is estimated at 6″.

Aitken 152, 157, 158. These three stars were all detected and measured before Aitken's results reached me.

The following stars I have been unable to find:

	h	m	°	′			
h 2373 ...	7	15·5	+56	21	(1880)	00 Jan.	27, Feb. 5.
h 1222 ...	12	46·8	+47	26	,,	02 Apr.	22
H*h* 495 ...	16	1·2	+42	20	,,	00 June	8
H*h* 635 ...	19	40·2	+37	15	,,	00 July	10
h 1481½ ...	20	2·6	+32	14	,,	00 Sept.	14
Mayer ...	21	4·0	+52	12	,,	00 Aug.	14
H*h* 724 ...	21	12·0	+37	16	,,	00 ,,	16
h 1773 ...	22	23·4	+58	17	,,	01 Jan.	5